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Carlos Henrique L. Corseuil, Rodrigo Leandro de. Moura

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The Effects Of Tax Incentives For Small Firms On Employment Levels*

Carlos Henrique L. Courseuil
IPEA (carlos.corseuil@ipea.gov.br)

Rodrigo Leandro de Moura
EPGE/FGV-RJ (rodrigoleandro@gmail.com)

Abstract

This paper will examine the effects of tax incentives for small businesses on employment level evaluating a program with this purpose implemented in Brazil in the 1990s. We first develop a theoretical framework which guides both the definition of the parameters of interest and their identification. Selection problems both into the treatment group and into the data sample are tackled by combining fixed effects methods and regression discontinuity design on alternative sub-samples of a longitudinal database of manufacturing firms. The results show that on the one hand the size composition of the treated firms may be changed due to the survival of some smaller firms that would have exited had it not been eligible to the program. On the other hand, the treated firms who do not depend on the program to survive do employ more workers.

Keywords: employment, tax incentives.

JEL Code: J21, H25.

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1 Introduction

This paper will examine the effects of tax incentives for small businesses on employment level. To do so we will study the tax incentives program for small businesses implemented in Brazil in the 1990s. The program, called SIMPLES, combines, simplifies and promotes the collection of federal taxes from micro-firms and small companies, with lower, though progressive, tax rates on the same base for calculation (gross revenues)¹. The program aims to improve the performance of the target establishments, in particular with a view to moving these establishments away from the informality and boost employment level.

Although frequently discussed among policymakers (see for example the World Bank [2009]), the question of using tax incentives to boost firms performance has not enjoyed the same support among academics. Existing studies focus on the impact of [fiscal incentive] programs on investment (Hasset and Hubbard [2002]; De Mooij and Ederveen [2003]; Klemm and Van Parys [2009]), leaving the question of the effect on employment open to discussion.

Another question frequently discussed among policymakers is the role of the small business in job creation. It is often said that this sector has a prominent role to play in employment generation and should therefore be the focus of incentive policies². There is a good deal of discussion about this issue among academic economists³ but little has been done to examine the possible impact of fiscal incentives on small businesses⁴.

Our study makes a contribution, therefore, by casting some light on a relevant question for public policies, which is: do small businesses respond to tax incentives with a change in the employment level?

Our work also presents some methodological contributions. The main contribution is in the empirical part of the paper where we were unable to use conventional methods to estimate the desired effect due to selection problems both

¹Note that SIMPLES combines reductions both in monetary and administrative costs of tax payment. Although the first dimension tends to be emphasized, Brasil (2009) provides evidences that the administrative costs are particular high in Brazil. For instance tax payment is responsible for 7% of the time devoted to administrative tasks in Brazilian firms as opposed to 4.1% in other Latin American countries.

²See for instance “Barack Obama and Joe Biden’s Plan for Small Business” available at www.barackobama.com/pdf/SmallBusinessFINAL.pdf

³On the one hand Birch (1987), and Newmark, Wall and Zhang (2008) find evidence supporting the importance of small firms in job creation on the other hand, Davis, Haltiwanger and Schuh (1996) present evidence that contradicts the ones presented by the authors above. Finally, Moscarini and Postel-Vinay (2009) show that the relative importance of small firms in the generation of employment varies according to the phase of the business cycle

⁴Lopez-Acevedo and Tinajero (2010) evaluates a bunch of programs oriented to small business in Mexico, but none of them is similar to the SIMPLES program

in the treatment group and in the data sample. We overcome both problems by combining fixed effects methods and regression discontinuity design on alternative sub-samples.

While the principal contribution of this paper may lie in the empirical front, we present a theoretical framework for the determination of employment at the firm level, and show how this process will be affected by the introduction and extension of the SIMPLES program. This theoretical analysis will turn out to be important as it reveals an heterogeneity of the effect with regard to the non-observed characteristics of the firm. This implies that we are able only to identify locally the parameter of interest. Moreover, the theoretical framework identifies two different components of the effect of tax incentives on employment. The first deals with the changes in employment due to the benefits of the program for firms that would survive even if the program had not been implemented. The other deals with an eventual change in the size composition of the treated firms derived from changes in the survival probability of these firms as they begin to experience the benefits of the program.

In the empirical part of the text we use longitudinal data from manufacturing firms located in Brazil to examine the contribution of the SIMPLES program to the average number of workers employed in each firm at two distinct points in time: in 1997, the year it was implemented; and again in 1999, when there was an increase in the revenue upper bound that determines the firm's eligibility for the program. Our strategy for identifying the effect of the SIMPLES program on employment decisions by firms is based on the comparison between the firms closest to the revenue upper bound for eligibility to the program. In a broad sense the idea is to contrast the employment level of those that opt for the program in relation to those that do not opt for the program. Our challenge was to identify each component of the effect in the presence of selection problem both, into the treatment group and into the data sample.

An important assumption to identify each one of the effects individually is the exogeneity of the revenue upper bound that defines eligibility for the program. One important aspect is that this value refers to the annual revenue in the previous year. So, for instance, when the program was implemented in 1997 (extended in 1999), firms' 1996 (1998) revenue were contrasted with the legal upper bound in order to define eligibility. Moreover, the 1997 bound of R\$ 720,000 was defined by law in December 1996 and was raised to R\$ 1,200,000 by a law passed in December 1998⁵. Therefore, we argue (and provide some evidence later) that along 1996 and

⁵It should be noted that the upper bounds of gross revenues for the definition of micro and small firms had already been determined previously by law in 1984 and 1994 respectively. The latter law defined the upper bound for a micro-firm as approximately R\$200,000 and for a small firm of approximately R\$600,000. Therefore the SIMPLES legislation simply raise the limit for the definition of small firms (which is the limit that we are interested in because it coincides with

1998 firms could not manipulate their revenue flow in order to match the eligibility criteria, since they would have learned about this criterion in the last month of the respective years.

The remainder of the article is structured in the following manner. The second section presents the theoretical framework. The third section deals with our strategies for the identification and estimation of each of the effects mentioned above. The fourth and fifth sections are dedicated to the presentation and discussion of the database and the empirical results, respectively. We conclude the article with a brief summary and some conclusions.

Finally, we would like to point out that in addition to the level of employment in existing firms and their survival probability, another relevant dimension for job creation is the rate of creation of new firms. This issue is not addressed in this article. The studies by Monteiro and Assunção (2009), and Fajnzylber, Maloney and Rojas (2009), while not focused on the effect of the SIMPLES incentives on employment, offer a side discussion on the related subject of how the SIMPLES incentives affect the rate of formal registration of Brazilian micro firms.

2 Theoretical Framework

In this section we describe our view of how companies make the decision regarding labor demand. We assume that the theoretical framework developed by Jovanovic (1982) performs this role in a satisfactory manner, given that some of the implications of this framework have been successfully tested in a variety of empirical studies⁶. After a brief discussion of the theory, we develop some predictions about how the SIMPLES incentives will affect decisions on labor demand.

2.1 Assumptions

Our version of the theoretical framework differs slightly from the original in order to allow the firm to vary the demand for labor rather than the quantity of output. This can be accomplished by assuming that: i) labor (ℓ) is the only factor of production, ii) the production function is concave and, iii) firms are wage takers (w)⁷. For convenience, we will assume a specific form for the production function

the amount that determines eligibility for the SIMPLES program) to R\$720,000. Nevertheless it should be noted that the 1994 law differs from the SIMPLES legislation, because the first institutes other benefits in the areas of management, labor, retirement and credit, while the latter law deals with tax and fiscal areas. Finally, note that the 1994 law failed to have any significant impact because most of its benefits needed further regulations that were never signed.

⁶See for example Sutton (1997), Caves (1998), and Bartelmas and Doms (2000).

⁷A similar adaptation was made by Pakes and Ericson (1998).

as specified below⁸:

$$f(\ell_t) = \ln(\ell_t + 1).$$

The introduction of the SIMPLES program to the framework can be done if we are willing to assume that the introduction of this program is equivalent to a reduction of taxes on the gross revenues of the firm⁹. Using these assumptions we can use the following equation to describe the current profits of the firm:

$$\pi_t = (1 - \tau).p_t.u_t(\theta).f(\ell_t) - w_t.\ell_t,$$

where p_t corresponds to the unit price of the only good produced in the economy and $\tau = \tau^s$ corresponds to the tax rate that applies when the firm opts to enroll in the SIMPLES program and $\tau = \tau^n$ otherwise.

The main hypothesis of the Jovanovic (1982) model is that profits are affected by the term u_t , which in turn is influenced by firms' innate efficiency level (θ).

Given the absence of costs of adjustment or any other factor that makes the choice of employment level an inter-temporal decision, the firm makes this decision to maximize the expected value of current profits. Another assumption that simplifies the problem is that p_t is known at the beginning of period t . However, suppose that the firm does not know the value of u_t it will be subjected to during t . What is known about u_t is the following:

- i. $u_t = \theta + v_t$
- ii. $v_t \stackrel{iid}{\sim} N(0, \sigma^2)$

In other words u_t could be interpreted as a signal revealed at the end of period t , related to firm's innate efficiency. The coincidence of these two variables is impeded by the noise component (v_t).

Returning to the decision about the optimal level of employment (ℓ^*), this decision will be based on the expectation of the firm with regard to u_t . Based on assumptions (i) and (ii) above, it is easy to deduce $E[u_t | \theta] = \theta$. However, the firm is also unaware of the true value of θ ¹⁰. The firm uses information from the

⁸The convenience of this assumption will be explained in the empirical section.

⁹Although the SIMPLES program in fact corresponds to taxes on gross revenues, this assumption simplifies the fact that some taxes replaced by the SIMPLES program do not fall on revenue, (for example contribution to social security). Moreover this assumption also ignores the effect of the simplification of tax forms and filing under SIMPLES. In other words, we are measuring jointly the effect of tax simplification and tax reduction stipulated in the SIMPLES legislation. Unfortunately it was not possible to identify separately these two effects due to data limitation.

¹⁰All that is known is that the distribution of this random variable across firms is normal and as a result, all firms have the same expectation with regard to their degree of efficiency at the beginning of their first period of activity.

signals that it has received throughout its period of activity to make a Bayesian inference about this random variable, such that the firm chooses the employment level by maximizing the following definition of expected profits::

$$\max_{\ell_t} E[\pi_t] = \max_{\ell_t} (1 - \tau) p_t \cdot E_t[\theta | \tilde{u}_{t-1}] \cdot \ln(\ell_t + 1) - w_t \cdot \ell_t$$

where $\tilde{u}_{t-1} = (u_{t-1}, \dots, u_1)$ is the history of signals that the firm has received from its inception up to the decision period. The optimal level of employment is given by:

$$\ell_t^* = \frac{(1 - \tau) p_t E_t[\theta | \tilde{u}_{t-1}]}{w_t} - 1 \quad (1)$$

Let T be a binary variable that shows whether the firm opts to enroll in the SIMPLES program. The relationship of this variable to the optimum employment level can be seen in its clearest form by rewriting the equation above as follows:

$$\ell_t^* = \frac{(1 - \tau^{ns}) p_t}{w_t} \cdot E_t[\theta | \tilde{u}_{t-1}] - 1 + T \cdot \frac{(\tau^{ns} - \tau^s) p_t}{w_t} \cdot E_t[\theta | \tilde{u}_{t-1}] \quad (2)$$

Another important decision taken by the firm is whether it should continue to operate. At the start of each period the firm decides to shut down (forever) or continue operating for another period. Note that this decision is inter-temporal in its nature given that what is decided in period t will have an impact on decisions that will be taken in $t + 1$. To simplify this decision is assumed that the firm is aware of the entire trajectory of price and wage levels. Under similar conditions, Jovanovic (1982) derived the following results:

- For every age (n) there is an upper bound of $E_t[\theta | \tilde{u}_{t-1}]$ that determines the permanence of the firm. We call this limit $\gamma(n; p_t, \tau)$. When $E[\theta | \tilde{u}_{t-1}]$ is found to be below this level the firm decides to cease its activities, and will remain in the market otherwise.

Hence, we can interpret ℓ_t^* as a latent variable and define the optimal level of employment as:

$$\ell_t^{obs} = \begin{cases} \ell_t^*, & E_t[\theta | \tilde{u}_{t-1}] > \gamma(n; p_t, \tau) \\ 0, & E_t[\theta | \tilde{u}_{t-1}] \leq \gamma(n; p_t, \tau) \end{cases} \quad (3)$$

2.2 Predictions about the Effects of SIMPLES

The equation (2) allows us to examine the effect of the SIMPLES program on latent employment (ℓ^*). As $(\tau^{ns} - \tau^s) > 0$ it follows that:

- The optimal latent employment level increases when the tax rate is reduced from τ^n to τ^s (which is to say when T changes from 0 to 1), all else remaining constant.

That means that the observed employment level should increase for firms that survive. However the surviving decision may also be affected by changes in tax rates. In fact, the following prediction is proved in the Appendix:

- The upper bound for $E_t[\theta|\tilde{u}_{t-1}]$, at which point the firm decides to shut down, $\gamma(n; p_t, \tau)$, diminishes when the tax rate is reduced from τ^n to τ^s , although other factors remaining constant. In other words, $\gamma(n; p_t, \tau^s) < \gamma(n; p_t, \tau^n)$.

A third prediction emerges naturally as a corollary to the prediction above:

- Some firms that decide to exit the market when subject to tax τ^n decide to remain, to the extent that they are subject to tax rate τ^s . These firms are those with $\gamma(n; p_t, \tau^s) < E_t[\theta|\tilde{u}_{t-1}] < \gamma(n; p_t, \tau^n)$. A corollary also follows from this prediction:
- The firms that close down their activities when subject to tax rate τ^n , but do not close down when subject to tax rate τ^s , tend to be smaller than those that survive when subject to either one of the two tax rates.

The prediction above is based on the fact that ℓ_t^* is directly proportional to $E_t[\theta|\tilde{u}_{t-1}]$, as shown in equation (1).

The predictions above describe the two components of the effect of SIMPLES, which are precisely defined below. First, we define the scale component as:

$$\alpha = E[\ell^* \mid \tau^s, E_t[\theta|\tilde{u}_{t-1}], E_t[\theta|\tilde{u}_{t-1}] > \gamma(\tau^n)] - E[\ell^* \mid \tau^n, E_t[\theta|\tilde{u}_{t-1}], E_t[\theta|\tilde{u}_{t-1}] > \gamma(\tau^n)] \quad (4)$$

The other will be referred to as the composition component, and can be defined as:

$$\beta = E[\ell^* \mid \tau^n, \gamma(\tau^n) > E_t[\theta|\tilde{u}_{t-1}] > \gamma(\tau^s)] - E[\ell^* \mid \tau^n, \gamma(\tau^n) < E_t[\theta|\tilde{u}_{t-1}]] \quad (5)$$

The interpretation of these two components is the following. First, firms that would have survived even if the program had not been implemented may change their employment level depending on their option to enroll in the program (this amount should increase according to our theoretical predictions above). Second, the mortality rates of firms that opt for the SIMPLES program may change as well, especially among the smaller firms, changing the composition of this group of firms with regard to firm size (the prediction above - suggests a lower mortality rate for small firms - decreasing the average size of the treated firms).

The overall effect encompasses both components. The identification and estimation of the overall effect and each of the components are discussed in the next section.

3 Empirical Strategy

Our strategy for identifying the impact of the SIMPLES program on employment decisions by firms is based on the comparison between the firms closest to the revenue upper bound for eligibility to the program. In a broad sense the idea is to contrast the employment level of those that opt for the program in relation to those that do not opt for the program in the first year of implementation (1997), as well as in the year in which the revenue upper bound for eligibility to the program was increased (1999).

As previously mentioned, we attempted to identify the overall effect and its two components, called scale and composition, separately. Briefly, the scale effect is related to the effect of entering the SIMPLES program on the level of employment of a firm that would have survived even if the program had not been implemented. The composition effect measures the variation in the average level of employment of firms that opt for the program due to the fact that this group also includes firms that would have closed their doors if the program had not been implemented.

3.1 Identification and Estimation of the Overall Effect

The definition of both components of the overall effect is based on the expectation of the firm with respect to its level of efficiency. This variable, however, is not observed by the investigators. However, we can use the following results¹¹:

- For a given τ , at a given point in time, $E[\theta|\tilde{u}_{-1}]$ determines a single ℓ_t^* and, also a unique value for sales ($p_t.f(\ell_t^*) = R_t$),
- $E_t[\theta|\tilde{u}_{t-1}] = \alpha_n.E_{t-1}[\theta|\tilde{u}_{t-2}] + (1 - \alpha_n).\bar{u}_{t-1}$,

to redefine the latent optimum level of employment as:

$$\ell_t^* = \gamma^1.R_{t-1} + \gamma^2.T.R_{t-1} + \gamma^3.\theta + \gamma^4.T.\theta + \gamma^5.\bar{v}_{t-1} + \gamma^6.T.\bar{v}_{t-1} \quad (6)$$

where, R_{t-1} is a proxy variable for $E_{t-1}[\theta|u_1...u_{t-2}]$ derived from the first result immediately above. It is important to note that the definition of u_t (property i, in section 2.1) was used in the expression above. An important result immediately derived from (6) is that θ becomes the driving force of firm's decision when conditioned by R_{t-1} . So it is easy to see that the survival decision can be represented comparing θ with another threshold ($\gamma'(\tau)$). So, as long we compare firms with similar revenue levels, any distinct decision either on survival or on the employment level reflects distinct values for θ .

¹¹The first result below can be derived easily from equation (1), and the second can be seen in DeGroot (2004), section 9.5.

We argue that by using the assumption that v_t is white noise (property ii in section 2.1), the overall effect of the SIMPLES program will be locally identified for specific values of R_{t-1} and theta θ by:

$$E[\Delta\ell_t^*|R_{t-1}, T = 1] - E[\Delta\ell_t^*|R_{t-1}, T = 0], \quad (7)$$

In order to see this, note that the first expectation above comprises two groups of firms: those that depend on SIMPLES for survival, and those that would survive in any case (with or without the program). Call ρ the share of the first group, then we can write:

$$E[\Delta\ell_t^*|R_{t-1}, T = 1] = \rho E[\Delta\ell_t^*|R_{t-1}, T = 1, \gamma'(\tau^s) < \theta < \gamma'(\tau^n)] \\ + (1 - \rho) E[\Delta\ell_t^*|R_{t-1}, T = 1, \theta > \gamma'(\tau^n)]$$

The decomposition above can be used to reach the following result stated above:

$$E[\Delta\ell_t^*|R_{t-1}, T = 1] - E[\Delta\ell_t^*|R_{t-1}, T = 0] = \\ E[\Delta\ell_t^*|R_{t-1}, T = 1, \theta > \gamma'(\tau^n)] - E[\Delta\ell_t^*|R_{t-1}, T = 0, \theta > \gamma'(\tau^n)] + \\ \rho \{E[\Delta\ell_t^*|R_{t-1}, T = 1, \gamma'(\tau^s) > \theta > \gamma'(\tau^n)] - E[\Delta\ell_t^*|R_{t-1}, T = 1, \theta > \gamma'(\tau^n)]\}$$

Therefore what we identify as the overall effect is the sum of the scale component with a weighted version of the composition component (weighted by the share of firms that depends on SIMPLES to survive). In order to be able to name the first term as the scale effect we need a mixture of assumptions for regression discontinuity and for differences-in-differences: the variations in the employment level of the firms that opted for the SIMPLES program, but would survive even if SIMPLES was not implemented, had they not opted should be equal to the variation of the employment level of the non-treated group, conditioned on a particular value of R_{t-1} . For reasons that will become clear later, we will condition the analysis on revenue values close to the upper bound for eligibility (c).

Using equation (6)., one can see that the overall effect depends on $E[\theta|.]$ and R_{t-1} :

$$E[\Delta\ell_t^*|R_{t-1}, T = 1] - E[\Delta\ell_t^*|R_{t-1}, T = 0] = \gamma^2.R_{t-1} + \gamma^4.E[\theta|R_{t-1}] \quad (8)$$

This result should be stressed since it suggests we are dealing with heterogeneous treatment effects, which means that our identification will be local.

The identification strategy discussed above suggests that the overall effect can be estimated by OLS in the following specification derived from equation (6)¹²:

$$\Delta\ell_t^* = \gamma^1.\Delta R_{t-1} + (\gamma^2.c + \gamma^4.\theta).T + \gamma^2.T.(R_{t-1} - c) + \gamma^5.\Delta\bar{v}_{t-1} + \gamma^6.T.\bar{v}_{t-1}. \quad (9)$$

¹²We added and subtracted $\gamma^1 c$ and $\gamma^2 c$ to obtain:

$$\ell_t^* = \gamma^1.c + \gamma^1.(R_{t-1} - c) + \gamma^2.c.T + \gamma^2.T.(R_{t-1} - c) + \gamma^3.\theta + \gamma^4.T.\theta + \gamma^5.\bar{v}_{t-1} + \gamma^6.T.\bar{v}_{t-1}$$

Then all we have to do is take the first difference to obtain (9).

Our principal interest lies in the coefficient associated with the dummy variable T , which captures the overall effect. In this estimation we had added a vector of control variables, that represent other determinants of the employment level not observed by the investigator. With regard to the observable variables, the theoretical framework described in section 2 suggests to us two types of variables to be considered. First, the age of the firm needs to be included since the critical values involved in both the decision to continue operating as well as the optimum size of the firm vary according to age. Further, the theory assumes that the firms are producing a homogeneous product, which suggests controls that define the same market and/or the use of the same technology. In this case we used indicators from the subsectors of the industry (sub), from the States (UF) in which the firm is operating, and an indicator of whether the firm is made up of a single establishment (uni) and dummy variables for the age of the firm (dage). More precisely, in the model (9) we have a variation of ℓ , and hence the controls that do not change over time are eliminated.

3.2 Identification and Estimation of the Composition Effect

It can be very informative to know the relative importance of the two components of the overall effect. In order to do this we should be able to identify (and estimate) at least one of the components individually. We claim in this section that it can be done with the composition component.

Then all we have to do is take the first difference to obtain (R_{t-1}, θ) it may be that some firms continue operating when subject to the conditions of SIMPLES, but would cease operations if there were no such program. These new survivors can change the distribution of the size of survivors in general (or of a subset of these) and, therefore, the average employment of the group under consideration. In the absence of data that show the option for the SIMPLES program for the firms that closed, this effect could be identified indirectly by exploring the discontinuity in the criteria for eligibility for the program.

The equation (efeitocomposicaoempirico) suggests that the identification of this component can be achieved by comparing the average employment of firms subject to the same tax rates, but differently positioned on the theta distribution depending on revenue. The difference with regard to whether the firm is positioned to the left or right of the upper bound that defines surviving under non-SIMPLES tax rate. We claim that the following difference in sample employment averages of firms surviving one year ahead of the employment measurement can be used to identify the composition effect:

$$\lambda = E \left[\ell_{(t-1)}^* | R_{t-1} = c^-, \theta > \gamma'(\tau^s) \right] - E \left[\ell_{(t-1)}^* | R_{t-1} = c^+, \theta > \gamma'(\tau^n) \right],$$

where $(t-1)'$ is the point in time immediately prior to the implementation of the SIMPLES program (December 31, 1996), or prior to the increase in the revenue upper bound for eligibility (December 31, 1998). The use of the equation above to identify the composition effect relies on a decomposition of the first term above in two groups analogous to the one implemented for the overall effect.

$$E \left[\ell_{(t-1)'}^* | R_{t-1} = c^-, \theta > \gamma'(\tau^s) \right] = (1 - \omega) E \left[\ell_{(t-1)'}^* | R_{t-1}, \gamma'(\tau^n) > \theta > \gamma'(\tau^s) \right] \\ + \omega E \left[\ell_{(t-1)'}^* | R_{t-1}, \theta > \gamma'(\tau^n) \right]$$

We can use the equation above to re-write λ as:

$$\lambda = E \left[\ell_{(t-1)'}^* | R_{t-1} = c^-, \theta > \gamma'(\tau^s) \right] - E \left[\ell_{(t-1)'}^* | R_{t-1} = c^+, \theta > \gamma'(\tau^n) \right] = \\ (1 - \omega) \left\{ E \left[\ell_{(t-1)'}^* | R_{t-1} = c^-, \gamma'(\tau^n) > \theta > \gamma'(\tau^s) \right] - E \left[\ell_{(t-1)'}^* | R_{t-1} = c^-, \theta > \gamma'(\tau^n) \right] \right\} + \\ \left\{ E \left[\ell_{(t-1)'}^* | R_{t-1} = c^-, \theta > \gamma'(\tau^n) \right] - E \left[\ell_{(t-1)'}^* | R_{t-1} = c^+, \theta > \gamma'(\tau^n) \right] \right\}$$

The equation above describes λ as the sum of two terms. The first corresponds to the composition component, while the second capture employment differences at the boundary for eligibility due to differences in the efficiency level (θ) net of the composition effect. We claim that the second term is null, which allows the use of λ to identify the composition component. To do so, we must rely on the assumption that firms have not manipulated their revenues in anticipation of the implementation/modification of the SIMPLES law. We provided some support for this hypothesis in the introduction.

We would like to stress two aspects of the equation above. First, the sample restriction of firms surviving one year after the decision period guarantees that the composition effect takes place. Second, the fact that we compare the firms in $(t-1)'$ means that they are all subject to the same pre-program environment, including the same tax rate.

The estimation of λ can be conducted through the following regression discontinuity model:

$$\ell_{(t-1)'}^* = \tilde{\gamma}^1 \cdot c + \tilde{\gamma}^1 \cdot (R_{t-1} - c) + \tilde{\gamma}^3 \cdot D + \tilde{\gamma}^4 \cdot D \cdot (R_{t-1} - c) + \tilde{\gamma}^5 \cdot \bar{v}_{t-1}, \quad (10)$$

where, D is used as a dummy variable for eligibility, equal to 1 if the firm has revenues that are lower than the upper bounds established by law. This dummy variable will capture the composition effect, or λ to be more precise. This can be easily derived from equation (6), taking into account that at $(t-1)'$, $T = 0$ for all firms.

4 The Data Base and Descriptive Statistics

The principal source of data is the Pesquisa Industrial Anual (PIA), an annual survey of business establishments conducted by the Brazilian Institute for Geography and Statistics (IBGE). We use the files for the period from 1996 to 1999. This data allows the combination of information regarding employment, enrollment in the SIMPLES program and gross revenues, and all of the other variables mentioned in the specification of the empirical model but the age of the firm. This last information, as a categoric variable, was taken from the *Relação Anual de Informações Sociais* (RAIS)¹³, since both of these databases use the same code for the identification of firms. It should, however, be pointed out that there are two restrictions that might keep a firm from showing up in the sample of the PIA in year “t”: (i) it must have appeared in the RAIS in period “t -1” and must have had more than five employees and (ii) in the case it had between five and 30 employees in period “t -1” it will be selected randomly to be part of the PIA in period “t”, and in the case where the firm has more than 30 employees in “t -1” it will be part of the sample for sure in period “t”. Hence the PIA does not include firms that were created in period “t”.

Therefore, we cannot take into account the impact on the creation of new firms in any of our estimations.

On Table 1, we can see that among the firms sampled in the PIA in 1997, 42,294 firms were eligible for the SIMPLES program and of these, 32,735 (77.4%) decided to opt for the SIMPLES program in 1997. This number shows that 1996 revenue, is in fact the most important criterion, or that the others are not quite so relevant. In addition, we note that there is some degree of measurement error in the eligibility variable, because there are firms that were not eligible for the program and identified themselves as having qualified to enter the SIMPLES program. This measurement error may reflect either error in the revenue declaration by firms or the negligence of other eligibility criteria. But the magnitude of these errors is small, because only 734 firms fall into this group^{14,15}.

¹³This database includes various data about the labor market for firms and the formal (“registered”) sector of the economy, in other words those firms that are registered as they are in the PIA.

¹⁴This occurs because, according to the legislation, the gross revenue considered as a criterion for eligibility should deduct canceled sales and unconditional discounts that have been granted. For part of the sample we have detailed information on these items.

We re-estimated all the models using deducting those items from the gross revenue for this part of the sample. The results do not change.

¹⁵There is another component of this measurement error that we were unable to observe: firms would’ve been eligible, but were classified as an eligible, and did not opt for the SIMPLES program. But since the majority of eligible firms opted for the SIMPLES program, this other source of measurement error probably is even more negligible, given that only 734 of the non-

In 1999, there was a significant increase in the number of firms eligible for, and choosing to enter, the SIMPLES program¹⁶, to 59,128 eligible firms and, of these 48,425 (81.9%) were (auto)selected into the program. This number shows that revenues, once again, is the most important criterion, or that the others are not so relevant, in addition, the measurement error in the eligibility variable observed in 1997, is reduced to a little more than 450 firms who opted for the SIMPLES program but were not eligible¹⁷.

Table 1: Frequency of Firms Opting for SIMPLES in 1997 and 1999 by Eligibility.

	Eligible				Total	
	No	(%)	Yes	(%)	Total	(%)
Opting for SIMPLES						
			1997			
No	20,838	32.63	9,554	14.96	30,391	47.59
Yes	734	1.15	32,735	51.26	33,470	52.41
Total	21,572	33.78	42,289	66.22	63,861	100
Opting for SIMPLES						
			1999			
No	17,199	22.4	10,703	13.94	27,902	36.34
Yes	453	0.59	48,425	63.07	48,878	63.66
Total	17,652	22.99	59,128	77.01	76,780	100

Note: Based on Data from the PIA.

Eligible if $0 \leq \text{gross revenue in 1996} \leq 720000$ and Non-Eligible if $\text{gross revenue in 1996} > 720000$ for 1997

Eligible if $0 \leq \text{gross revenue in 1998} \leq 1200000$ and Non-Eligible if $\text{gross revenue in 1998} > 1200000$ for 1999

Figures 1 and 2¹⁸ below show that: (i) the percentage of firms opting for the SIMPLES program is extremely high for low levels of income and (ii) the measurement error is more concentrated close to the upper bound value that defines eligibility. There is, therefore, a large discontinuity around c for both of the years analyzed. So is there an impact on the level of employment around c ?

eligible firms opted for the SIMPLES program.

¹⁶The increase in eligible firms in absolute and relative terms is due to the increase in the upper revenue bound from R\$720,000 to R\$1,200,000, which made it possible for a larger number of firms to become eligible.

¹⁷In the following section, we will present some checks for robustness of the results showing that the measurement error did not affect the results obtained.

¹⁸A point on Figure 1, represents the percentage of firms choosing to enter the SIMPLES program within a given revenue bin. Since we are working with a large sample of firms, we calculated the percentage of firms choosing to enter the SIMPLES program (Figure 1), by revenue bins of R\$15,000. The same bins were applied in the calculation of the average number of employees (Figures 3 and 2) and for the frequencies related to some control variables (Figures 5 – 8).

Figure 1: Percentage of Firms Opting for SIMPLES In 1997 By 1996 Revenue

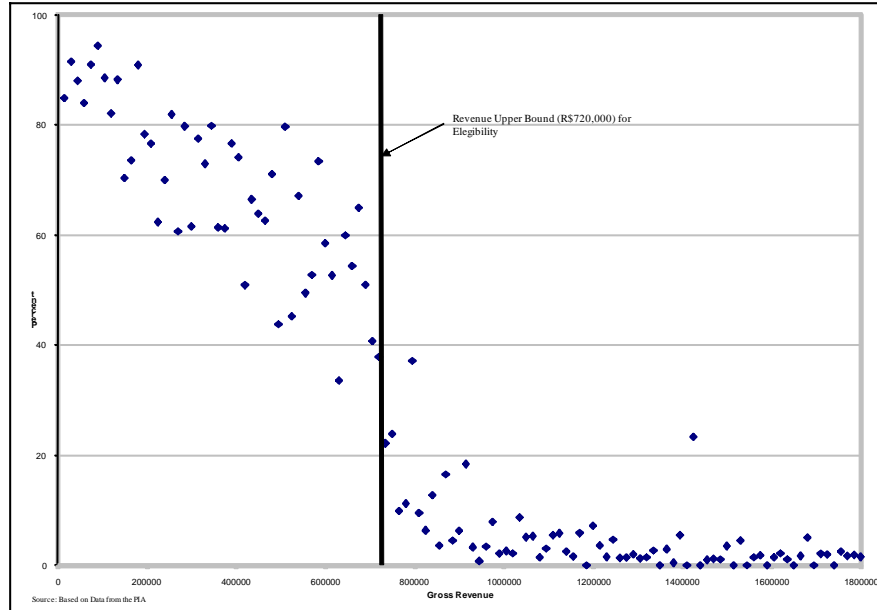
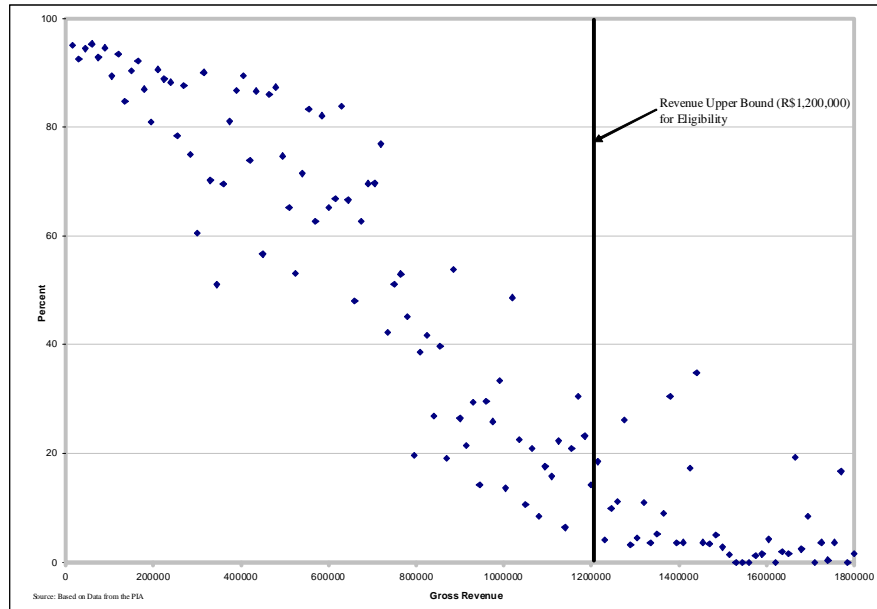


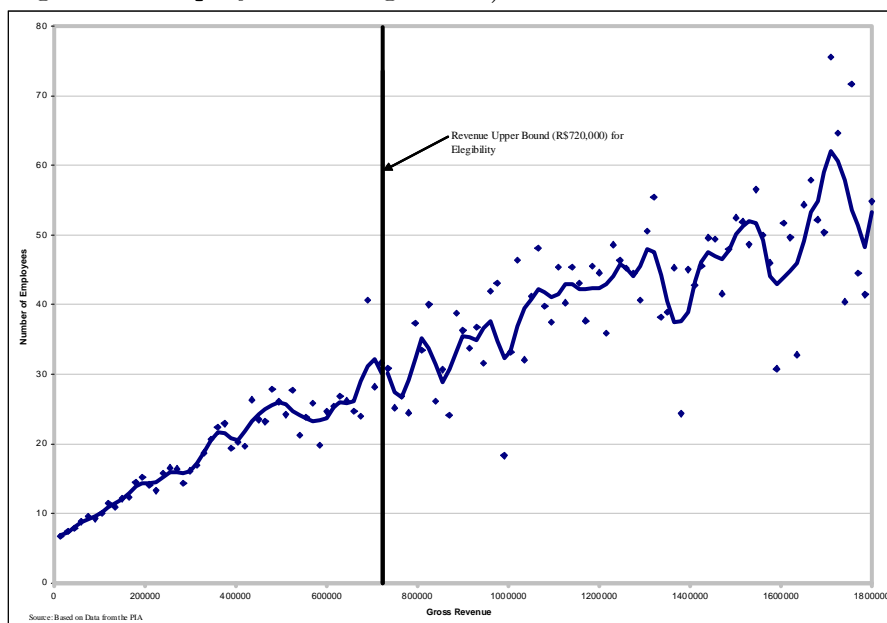
Figure 2: Percentage of Firms Opting for SIMPLES In 1999 By 1998 Revenue



Figures 3 and 4 attempt to answer the following question. Is there a discontinuity in the number of employees for firms with revenues around the revenue threshold? Initially, we found that firms with a higher level of income tended to have a larger number of employees, which was expected. However it is important to point out an interesting aspect: the firms that were eligible for the SIMPLES program (in other words, firms with revenue lower than the vertical solid line—below R\$ 720,000 in 1996 and R\$ 1,200,000 in 1998) show lower dispersion in terms of the number of employees then do the non-eligible firms.

In addition, we estimated a kernel-weighted local polynomial regression¹⁹ (continuous line) suggesting that the impact on the employment level around the revenue upper bound is nil for 1997 and favors the non-eligible firms (those with income greater than R\$ 1,200,000) in 1999. Nevertheless it is important to point out that this impact is not controlled by the selection mechanisms as indicated above, as well as the other factors captured by the control variables.

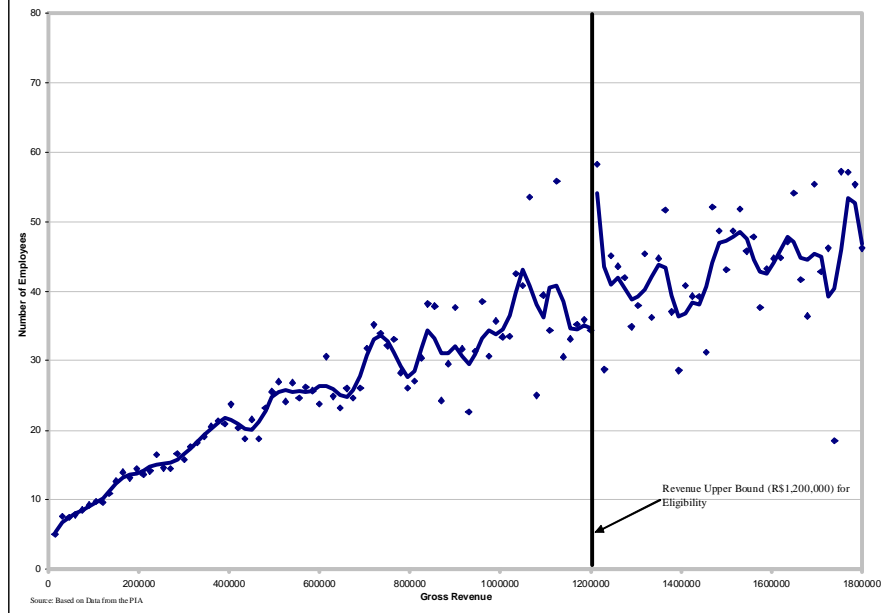
Figure 3: Average Number of Employees in 1997 By 1996 Revenue (solid line: kernel-weighted local polynomial regression)



Given the evidence on the figures above, we may still ask if there are any other factors that showed discontinuous variations around c . In principle, there are not

¹⁹More specifically, we estimated a local linear regression, employing Epanechnikov kernel with a R\$15,000 bandwidth.

Figure 4: Average Number of Employees in 1999 By 1998 Revenue (solid line: kernel-weighted local polynomial regression)



as can be seen in figures 5 and 8. We note that on figures 5 and 6 the proportion of firms with only one establishment is quite high for the lower levels of revenues. Moreover, it is important to stress that around the revenue upper bound, the proportion of firms with only a single establishment changes very little. Finally, on Figures 7 and 8 the proportion of older firms with more time in operation (more than 10 years) is much higher than for newer firms, principally, those with higher revenues. We would again point out that around the eligibility upper bound, the frequency of firms within each age level is similar.

Thus, the evidences presented in the last four Figures show that the control variables do not differ greatly for firms that are near the revenue upper bound.

5 Results Of the Regressions

In this section we present the estimates from the regression models (9) and (10), that attempt to identify the overall and composition components of the effect of the SIMPLES program on average levels of employment for firms with revenues close to R\$ 720,000 in 1996 and R\$ 1,200,000 in 1998. The limits of what is

Figure 5: Percentage of Firms With Only One Establishment In 1997 By 1996 Revenue

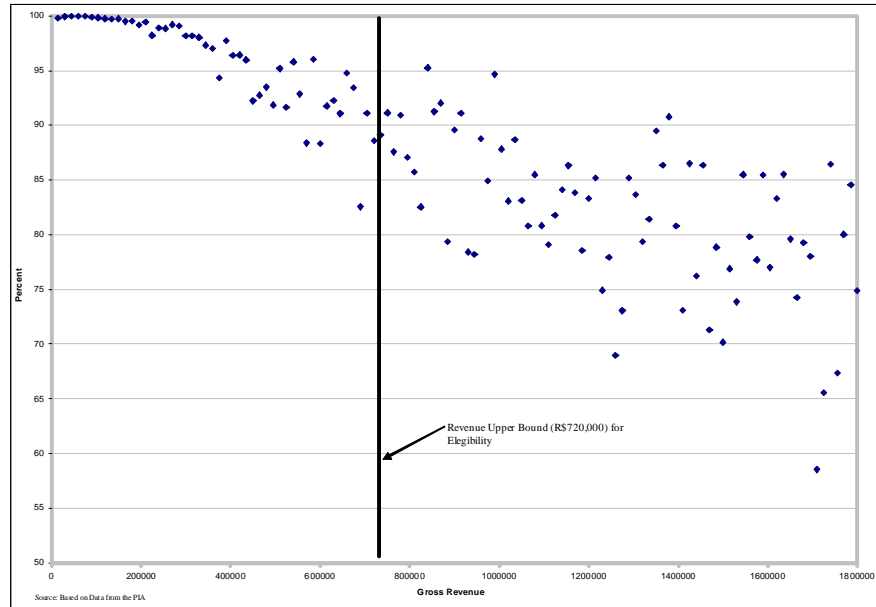


Figure 6: Percentage of Firms With Only One Establishment In 1999 By 1998 Revenue

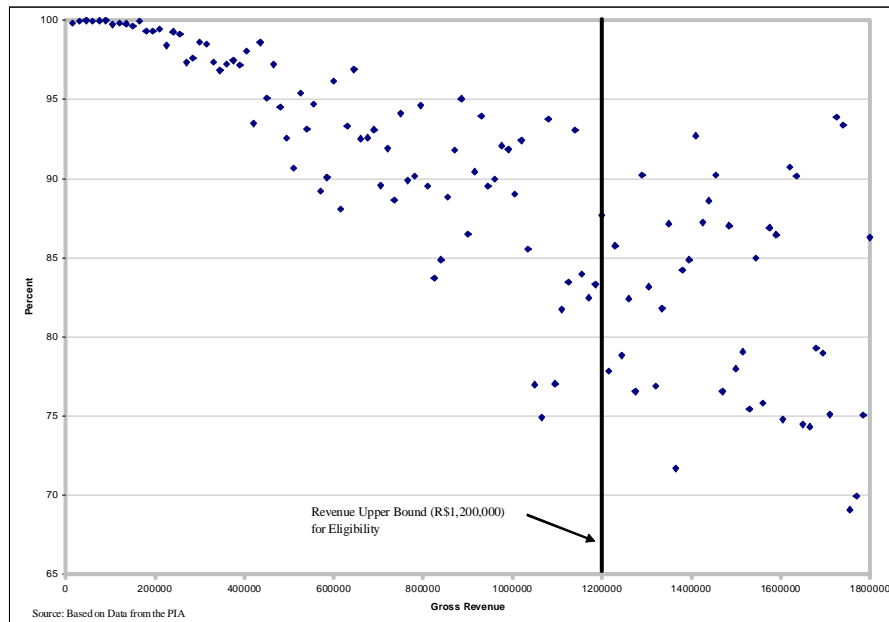


Figure 7: Distribution of Firms Across Age Categories in 1997 By 1996 Revenue

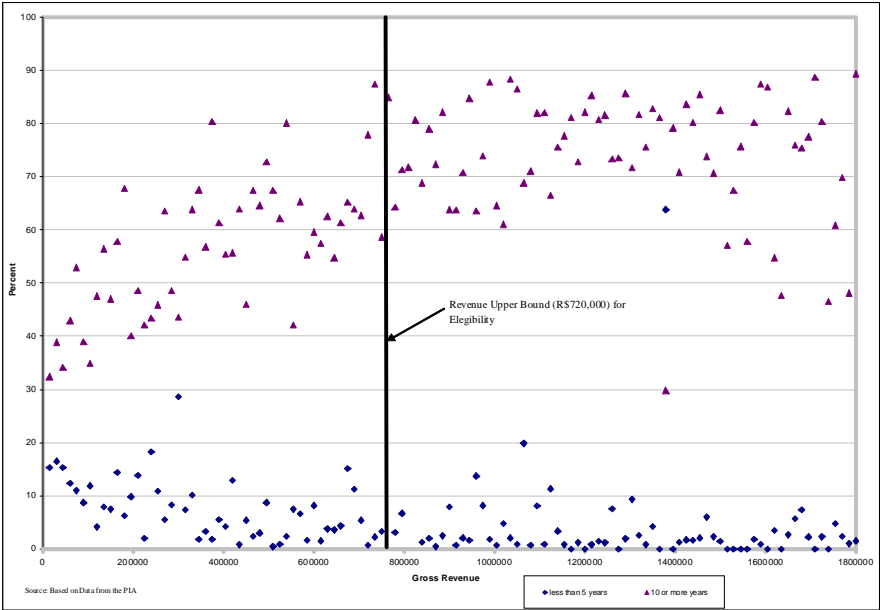
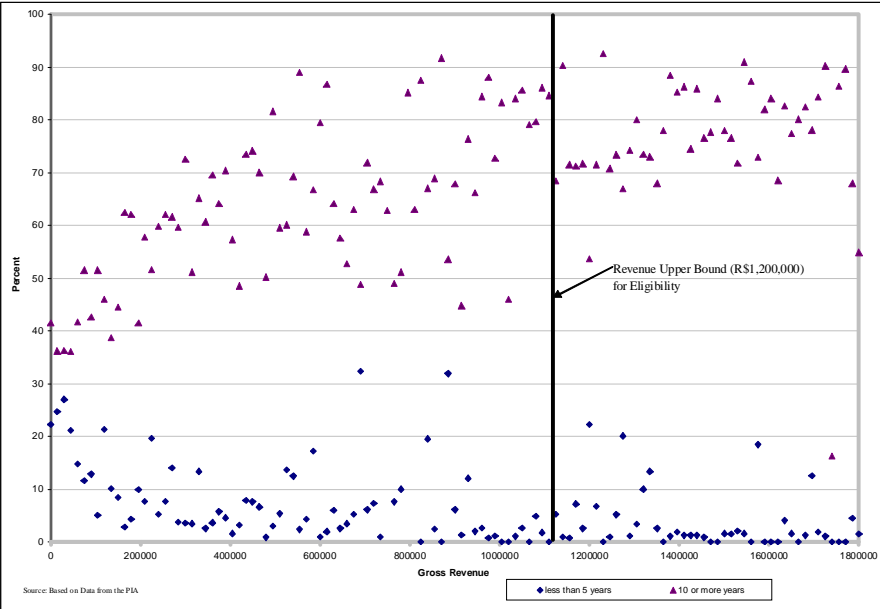


Figure 8: Distribution of Firms Across Age Categories in 1999 By 1998 Revenue



considered close are provided by an arbitrary bandwidth. Thus, for example, if the bandwidth is equal to R\$ 300,000 we are restricting the sample for firms with revenues between (R\$ 720,000 - R\$ 300,000) and (R\$ 720,000 + 300,000).

There is still no consensus in the literature about the definition of the optimum size of the bandwidth. In practical terms, Imbens and Lemieux (2008) suggest that the regression model should be estimated varying the bandwidth size, in other words, taking samples with different values for h . In principle, the smaller the bandwidth, the better the estimation accuracy, provided that it has a sufficiently large number of observations. We considered bandwidth “sizes” of R\$ 50,000 to R\$ 300,000, in R\$ 50,000 increments. In addition, we estimated models using a rule-of-thumb (ROT) bandwidth as suggested by Lee and Lemieux (2009), and used in empirical studies (McCrary and Royer, 2003), in order to compare the results with other bandwidths.

The results of the model (9), estimated using bandwidths around c , are shown on Tables 2 and 3 below. For 1996/97 (Table 2), we observe that the impact of the SIMPLES program on changes in the employment level in treated firms is positive and significant for all bandwidths, with the exception of the smaller (R\$50,000), possibly due to a small sample size. In terms of magnitude, the coefficient does not vary significantly among the bandwidths considered, but there is an increase in the employment level of approximately 1.3 to 2 additional employees when the firm chooses to enroll in the SIMPLES program. Therefore the overall effect, measured by the equation (8) is positive. In relative terms the impact represents from 5% to 7% of the average employment level in the sample, which is around 27 employees for alternative bandwidths.

For 1999, we observe that the impact of the SIMPLES program on changes in the employment level is positive and significant in the three largest bandwidths, and is positive but not significant in the remaining bandwidths, possibly due to the small sample size. In terms of magnitude, the coefficient does not vary significantly among the three largest bandwidths considered, with an increase in the employment level of approximately 2.2 to 2.8 additional employees when the firm chooses to opt for the SIMPLES program, in other words, from 6% to 7.5% of the average employment level in the sample, which was around 37 employees in 1999. Therefore, we have evidence of a positive effect that is somewhat weaker than in 1997.

Table 2. Estimates for the Overall Effect of SIMPLES – 1996/97

Dependent Variable							
Change in the Number of Employees between 1996 and 1997							
	Bandwidth (in thousands)						
Regressors	300	250	200	150	100	50	h_{ROT}
Simples (T_{197})	1.69 <i>0.80</i>	1.96 <i>0.85</i>	1.64 <i>0.79</i>	1.29 <i>0.76</i>	1.86 <i>0.87</i>	1.80 <i>1.20</i>	1.77 <i>0.95</i>
ΔF_{196}	1E-08 <i>5E-09</i>	2E-08 <i>5E-09</i>	9E-09 <i>5E-09</i>	8E-09 <i>4E-09</i>	7E-10 <i>3E-09</i>	-7E-07 <i>2E-06</i>	-1E-09 <i>4E-09</i>
$D_{197} (F_{196} - c)$	-1E-06 <i>4E-06</i>	-1E-06 <i>7E-06</i>	9E-06 <i>7E-06</i>	-1E-06 <i>7E-06</i>	-1E-05 <i>2E-05</i>	-2E-06 <i>5E-05</i>	-3E-06 <i>2E-05</i>
Constant	-0.88 <i>1.01</i>	-1.18 <i>1.12</i>	-1.80 <i>1.14</i>	-0.88 <i>1.29</i>	-2.17 <i>1.77</i>	-2.59 <i>2.22</i>	-2.64 <i>2.08</i>
Number of Observations	3499	2880	2312	1728	1165	603	1022
Note: Included as dummy regressors for subsectors of the industry and for states. Simples (T_{197}) = 1 if the firm opted for SIMPLES in 1997 and 0 if it did not; ΔF_{196} is the change in gross revenue between 1995 and 1996; ($F_{196} - c$) = (Gross Revenue in 1996)-720000; h_{ROT} = rule of thumb bandwidth = 84.453 (in thousands) Standard Error in <i>italics</i> .							

Table 3. Estimates for the Overall Effect of SIMPLES – 1998/99

Dependent Variable							
Change in the Number of Employees between 1998 and 1999							
	Bandwidth (in thousands)						
Regressors	300	250	200	150	100	50	h_{ROT}
Simples (T_{199})	2.77 <i>1.34</i>	2.24 <i>1.14</i>	2.82 <i>1.15</i>	0.98 <i>1.07</i>	0.69 <i>1.25</i>	2.52 <i>2.11</i>	0.72 <i>1.18</i>
ΔF_{198}	3E-09 <i>1E-06</i>	6E-08 <i>1E-06</i>	1E-06 <i>8E-07</i>	6E-08 <i>8E-07</i>	5E-07 <i>1E-06</i>	2E-07 <i>1E-06</i>	1E-06 <i>1E-06</i>
$D_{199} (F_{198} - c)$	-6E-06 <i>3E-06</i>	-6E-06 <i>5E-06</i>	-2E-05 <i>7E-06</i>	4E-06 <i>1E-05</i>	7E-06 <i>2E-05</i>	3E-05 <i>5E-05</i>	1E-05 <i>2E-05</i>
Constant	0.64 <i>2.72</i>	0.66 <i>2.65</i>	-0.83 <i>2.07</i>	0.00 <i>4.03</i>	-5.54 <i>2.56</i>	-6.31 <i>3.30</i>	5.17 <i>4.37</i>
Number of Observations	2111	1734	1394	1054	740	393	794
Note: Included as dummy regressors for subsectors of the industry and for states. Simples (T_{199}) = 1 if the firm opted for SIMPLES in 1999 and 0 if it did not; ΔF_{198} is the change in gross earnings between 1997 and 1998; ($F_{198} - c$) = (Gross Revenue in 1998)-1200000; h_{ROT} = rule of thumb bandwidth = 103.795 (in thousands) Standard Error in <i>italics</i> .							

We now consider the composition effect, estimated using equation (10). The results are shown on Tables 4 and 5 below. For 1997, we observed that the composition effect was nil, in other words, for the year that the law was implemented, the SIMPLES program did not contribute to alter the size composition of the treated

group. However for 1999, we estimate a negative affect, that confirms our theoretical prediction that SIMPLES program made it possible for firms with lower efficiency levels to survive under the improved conditions of the program.

Table 4. Estimates for the Composition Effect of SIMPLES - 1996/97

Dependent Variable							
Number of Employees in 1996							
	Bandwidth (in thousands)						
Regressors	300	250	200	150	100	50	h_{ROT}
Eligibility (D_{i96})	-3.21 <i>2.80</i>	0.91 <i>1.91</i>	0.82 <i>2.04</i>	1.72 <i>2.28</i>	3.59 <i>2.43</i>	3.87 <i>3.26</i>	3.30 <i>2.54</i>
$(F_{i96} - c)$	-5.E-06 <i>2.E-05</i>	3.E-05 <i>1.E-05</i>	2.E-05 <i>1.E-05</i>	-7.E-06 <i>2.E-05</i>	7.E-05 <i>3.E-05</i>	2.E-05 <i>8.E-05</i>	5.E-05 <i>4.E-05</i>
$D_{i96} (F_{i96} - c)$	2.E-05 <i>2.E-05</i>	3.E-06 <i>2.E-05</i>	8.E-06 <i>2.E-05</i>	8.E-05 <i>2.E-05</i>	-4.E-06 <i>4.E-05</i>	6.E-05 <i>1.E-04</i>	2.E-05 <i>6.E-05</i>
Uni	-21.45 <i>1.94</i>	-20.40 <i>2.16</i>	-19.60 <i>2.28</i>	-19.45 <i>1.50</i>	-15.40 <i>1.77</i>	-12.48 <i>2.08</i>	-15.37 <i>1.79</i>
dage1	2.98 <i>1.84</i>	1.85 <i>2.19</i>	0.40 <i>1.95</i>	1.79 <i>2.31</i>	3.17 <i>3.07</i>	3.25 <i>3.22</i>	3.28 <i>2.94</i>
dage2	-0.81 <i>1.17</i>	-1.64 <i>1.35</i>	-2.08 <i>1.38</i>	-1.71 <i>1.42</i>	-1.53 <i>1.53</i>	-1.25 <i>2.10</i>	-1.33 <i>1.63</i>
Constant	43.50 <i>3.50</i>	39.68 <i>2.92</i>	40.17 <i>3.02</i>	41.21 <i>2.67</i>	35.27 <i>3.07</i>	35.23 <i>4.94</i>	37.38 <i>3.25</i>
Number of Observations	3501	2882	2313	1728	1165	603	1022

Note: Included as dummy regressors for subsectors of the industry and for states.

$D_{i96} = 1$, firm is eligible for SIMPLES in 1997 if $0 \leq (\text{Gross Revenue in 1996}) \leq 720000$ e $D_{i96} = 0$ if $(\text{Gross Revenue in 1996}) > 720000$.

$(F_{i96} - c) = (\text{Gross Revenue in 1996}) - 720000$;

Uni=1 if the firm has only one establishment, and 0 if it has more than 1;

Dage1=1 if the firm is less than 5 years old and Dage2=1 if the firm is more than 5 but less than 10 years old.

h_{ROT} = rule of thumb bandwidth = 84.453 (in thousands)

Standard Error in *italics*.

Table 5. Estimates for the Composition Effect of SIMPLES - 1998/99

Dependent Variable							
Number of Employees in 1998							
	Bandwidth (in thousands)						
Regressors	300	250	200	150	100	50	h_{ROT}
Eligibility (D_{198})	-6.25 <i>2.48</i>	-7.83 <i>2.61</i>	-7.46 <i>3.03</i>	-8.57 <i>3.47</i>	-7.59 <i>4.13</i>	-13.55 <i>5.87</i>	-8.81 <i>3.93</i>
$(F_{198} - c)$	-1.E-05 <i>1.E-05</i>	-3.E-05 <i>1.E-05</i>	-4.E-05 <i>2.E-05</i>	-3.E-05 <i>3.E-05</i>	-2.E-05 <i>5.E-05</i>	-3.E-04 <i>1.E-04</i>	-6.E-05 <i>5.E-05</i>
$D_{198} (F_{198} - c)$	3.E-05 <i>1.E-05</i>	3.E-05 <i>2.E-05</i>	7.E-05 <i>3.E-05</i>	4.E-05 <i>4.E-05</i>	1.E-05 <i>7.E-05</i>	3.E-04 <i>2.E-04</i>	5.E-05 <i>7.E-05</i>
Uni	-10.35 <i>1.54</i>	-10.20 <i>1.69</i>	-10.07 <i>1.82</i>	-10.59 <i>2.08</i>	-8.60 <i>2.47</i>	-7.40 <i>3.90</i>	-9.03 <i>2.25</i>
Dage1	-3.62 <i>2.28</i>	-2.60 <i>2.41</i>	-3.37 <i>2.73</i>	-5.15 <i>3.34</i>	-3.75 <i>4.30</i>	-15.00 <i>4.53</i>	-3.70 <i>4.24</i>
Dage2	-3.61 <i>1.61</i>	-2.39 <i>1.84</i>	-2.46 <i>1.99</i>	-0.04 <i>2.16</i>	-0.83 <i>2.48</i>	-4.66 <i>3.33</i>	0.03 <i>2.36</i>
Constant	46.58 <i>2.71</i>	49.12 <i>3.00</i>	50.36 <i>3.49</i>	50.32 <i>3.90</i>	48.28 <i>4.72</i>	53.96 <i>7.19</i>	47.60 <i>4.15</i>
Number of Observations	2442	2023	1627	1234	871	463	935

Note: Included as dummy regressors for subsectors of the industry and for states.

$D_{198} = 1$, firm is eligible for SIMPLES in 1999 if $0 \leq (\text{Gross Revenue in 1998}) \leq 1200000$ e $D = 0$ and $(\text{Gross Revenue in 1998}) > 1200000$.

$(F_{198} - c) = (\text{Gross Revenue in 1998}) - 1200000$;

Uni=1 if the firm has only one establishment, and 0 if it has more than 1;

Dage1=1 if the firm is less than 5 years old and Dage2=1 if the firm is more than 5 but less than 10 years old.

h_{ROT} = rule of thumb bandwidth = 103.795 (in thousands)

Standard Error in *italics*.

Thus, the evidence presented here suggests that in 1999 firms with revenues close to the revenue upper bound benefited from the SIMPLES program in two different ways. First, those that did not need the program to survive increased their employment level. Second, some less efficient firms, that would have shut down had the program not been implemented, remained active thanks to the benefits of the program. The evidence for 1997 suggests that only the first effect took place²⁰. Therefore we can say that these results are compatible with our theoretical predictions.

The Robustness of Results We also attempted to evaluate the robustness of the results in two dimensions: (i) by re-estimating the models with more flexible

²⁰Concerning the magnitude of our estimated values, there are reasons to believe that both the scale and composition effects may be sub-estimated. Concerning the scale effect it is possible that firms near the threshold may decide not to grow too much to avoid to become ineligible for the program in the next period. Concerning the composition effect, it is possible that the size composition of the treated group could also be modified by the entrance of new firms that would not enter if SIMPLES had not been implemented.

specifications, and (ii) “correcting” the measurement error related to a small fraction of firms being enrolled in the SIMPLES program even though they were not eligible. The procedure in this case was to impute a revenue value for these firms just below the threshold²¹.

Tables A1 – A4 present the evidence from both models for various specifications: linear (presented above), quadratic and cubic. In the vast majority of models the significance (or non-significance) of the coefficients remained unchanged. In quantitative terms, for the models that estimate the overall effect, the coefficients remain quite close (Tables A1 – A2). For the models that estimate the composition effect, the coefficients tend to be non-significant in the majority of cases for 1997 (Table A3) and statistically negative in almost all of these specifications for 1999 (Table A4)²².

Tables A5 – A6 show the evidence for the overall effect applying the procedure to deal with the measurement error, in other words, we change the values for the dummy variable D , from 0 to 1, for the firms that opted to enter the SIMPLES program ($T = 1$) and changed their gross income which is less than R\$ 720,000 (so that the firm will receive $D = 0$) to R\$ 720,000²³. The evidence confirms the results of the baseline specification.

6 Conclusions

In this paper we have estimated the employment effect of a tax incentive program that targets small business in Brazil. There are two main contributions in this paper. The first one was to carefully define the parameters of interest based on a theoretical model of labor demand. In doing so, we revealed two distinct transmission mechanisms, which we named as scale component and composition component.

The other contribution was to develop alternative strategies to identify and estimate the parameters of interest in the presence of selection, both into the data sample and into the program. Using longitudinal firm data we provide some evidence in line with our theoretical predictions.

The results show that on the one hand average employment tends to fall among firms that opt for the SIMPLES program, because of the fact that the program managed to avoid the exit of firms in 1999 that were relatively smaller. On the

²¹As previously stated in note 12 we also implemented an alternative procedure to deal with measurement error. The results are similar and will not be shown in this section, but are available upon request.

²²Only for the two smaller subsamples (100 and 50) is the effect greater in absolute terms, capturing some stronger non-linearity, closer to c .

²³Note that this procedure is not necessary for the estimation of the composition effect, because at $(t - 1)'$, the firms did not yet have the SIMPLES option..

other hand, the firms that choose to enroll in the SIMPLES program took advantage of the improved conditions under the program to employ more workers, both in 1997 as well as in 1999.

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7 Appendices

Table A1. Estimates for the Overall Effect of SIMPLES - Alternative Specifications -1996/97

Dependent Variable							
Change in the Number of Employees between 1996 and 1997							
	Bandwidth (in thousands)						
Regressor: Simple (T₁₉₇)	300	250	200	150	100	50	h _{ROT}
Linear	1.69 <i>0.80</i>	1.96 <i>0.85</i>	1.64 <i>0.79</i>	1.29 <i>0.76</i>	1.86 <i>0.87</i>	1.80 <i>1.20</i>	1.77 <i>0.95</i>
Quadratic	1.87 <i>0.81</i>	2.27 <i>0.90</i>	1.57 <i>0.81</i>	1.09 <i>0.75</i>	1.75 <i>0.88</i>	1.80 <i>1.20</i>	1.59 <i>0.96</i>
Cubic	1.94 <i>0.85</i>	2.17 <i>0.90</i>	1.49 <i>0.79</i>	0.98 <i>0.74</i>	1.67 <i>0.89</i>	1.59 <i>1.31</i>	1.60 <i>0.98</i>
Number of Observations	3499	2880	2312	1728	1165	603	1022
Note: Included as dummy regressors for subsectors of the industry, states, single establishment and age. Linear Specification uses: $\Delta F_{96} \cdot e_{D_{197}} \cdot (F_{196} - c)$ Quadratic Specification uses: $(F_{196} - c)^2 \cdot (F_{195} - c)^2 \cdot e_{D_{197}} \cdot (F_{196} - c)^2$ Quadratic Specification uses: $(F_{196} - c)^3 \cdot (F_{195} - c)^3 \cdot e_{D_{197}} \cdot (F_{196} - c)^3$ h _{ROT} = rule of thumb bandwidth = 84.453 (in thousands) Standard Error in <i>italics</i> .							

Table A2. Estimates for the Overall Effect of SIMPLES - Alternative Specifications -1998/99

Dependent Variable							
Change in the Number of Employees between 1998 and 1999							
	Bandwidth (in thousands)						
Regressor: Simple (T₁₉₉)	300	250	200	150	100	50	h _{ROT}
Linear	2.77 <i>1.34</i>	2.24 <i>1.14</i>	2.82 <i>1.15</i>	0.98 <i>1.07</i>	0.69 <i>1.25</i>	2.52 <i>2.11</i>	0.72 <i>1.18</i>
Quadratic	2.54 <i>1.24</i>	2.03 <i>1.10</i>	2.80 <i>1.08</i>	1.03 <i>1.08</i>	0.97 <i>1.26</i>	2.70 <i>2.11</i>	0.93 <i>1.20</i>
Cubic	2.64 <i>1.22</i>	2.36 <i>1.07</i>	2.67 <i>1.06</i>	1.21 <i>1.10</i>	1.12 <i>1.25</i>	2.63 <i>2.12</i>	1.18 <i>1.20</i>
Number of Observations	2111	1734	1394	1054	740	393	794
Note: Included as dummy regressors for subsectors of the industry, states, single establishment and age. Linear Specification uses: $\Delta F_{98} \cdot e_{D_{199}} \cdot (F_{198} - c)$ Quadratic Specification uses: $(F_{198} - c)^2 \cdot (F_{197} - c)^2 \cdot e_{D_{199}} \cdot (F_{198} - c)^2$ Quadratic Specification uses: $(F_{198} - c)^3 \cdot (F_{197} - c)^3 \cdot e_{D_{199}} \cdot (F_{198} - c)^3$ h _{ROT} = rule of thumb bandwidth = 103.795 (in thousands) Standard Error in <i>italics</i> .							

Table A3. Estimates for the Composition Effect of SIMPLES - Alternative Specifications - 1996/97

Variável Dependente

Number of Employees in 1996	Bandwidth (in thousands)						
Regressor: Elegibilidade (D₁₉₆)	300	250	200	150	100	50	h _{ROT}
Linear	-3.21 <i>2.80</i>	0.91 <i>1.91</i>	0.82 <i>2.04</i>	1.72 <i>2.28</i>	3.59 <i>2.43</i>	3.87 <i>3.26</i>	3.30 <i>2.54</i>
Quadratic	3.30 <i>2.81</i>	2.13 <i>2.91</i>	1.54 <i>3.01</i>	4.66 <i>3.22</i>	5.90 <i>3.22</i>	-0.21 <i>4.24</i>	7.39 <i>3.32</i>
Cubic	5.65 <i>3.49</i>	4.20 <i>3.35</i>	6.56 <i>3.49</i>	9.45 <i>3.52</i>	4.19 <i>3.68</i>	4.78 <i>5.66</i>	0.52 <i>4.10</i>
Number of Observations	3501	2882	2313	1728	1165	603	1022

Note: Included as dummy regressors for subsectors of the industry, states, single establishment and age.

D₁₉₆ = 1, firm is eligible for SIMPLES in 1997 if 0 ≤ (Gross Revenue in 1996) ≤ 720000 and D₁₉₆ = 0 if (Gross Revenue in 1996) > 720000.

Linear Specification uses: (F₁₉₆ - c) e D₁₉₆ · (F₁₉₆ - c)

Quadratic Specification uses: (F₁₉₆ - c)² e D₁₉₆ · (F₁₉₆ - c)²

Cubic Specification uses: (F₁₉₆ - c)³ e D₁₉₆ · (F₁₉₆ - c)³

h_{ROT} = rule of thumb bandwidth = 84.453 (in thousands)

Standard Error in *italics*.

Table A4. Estimates for the Composition Effect of SIMPLES - Alternative Specifications - 1998/99

Number of Employees in 1998	Bandwidth (in thousands)						
Regressor: Elegibilidade (D₁₉₈)	300	250	200	150	100	50	h _{ROT}
Linear	-6.25 <i>2.48</i>	-7.83 <i>2.61</i>	-7.46 <i>3.03</i>	-8.57 <i>3.47</i>	-7.59 <i>4.13</i>	-13.55 <i>5.87</i>	-8.81 <i>3.93</i>
Quadratic	-9.82 <i>3.48</i>	-7.09 <i>3.95</i>	-10.10 <i>4.52</i>	-8.33 <i>5.08</i>	-11.48 <i>6.54</i>	-17.64 <i>7.66</i>	-11.43 <i>6.26</i>
Cubic	-6.78 <i>4.71</i>	-9.22 <i>5.07</i>	-10.72 <i>5.89</i>	-16.27 <i>6.34</i>	-19.63 <i>7.43</i>	-20.82 <i>9.25</i>	-18.28 <i>7.46</i>
Number of Observations	2442	2023	1627	1234	871	463	935

Note: Included as dummy regressors for subsectors of the industry, states, single establishment and age.

D₁₉₈ = 1, firm is eligible for SIMPLES in 1999 if 0 ≤ (Gross Revenue in 1998) ≤ 1200000 and D₁₉₈ = 0 and (Gross Revenue in 1998) > 1200000.

Linear Specification uses: (F₁₉₈ - c) e D₁₉₈ · (F₁₉₈ - c)

Quadratic Specification uses: (F₁₉₈ - c)² e D₁₉₈ · (F₁₉₈ - c)²

Cubic Specification uses: (F₁₉₈ - c)³ e D₁₉₈ · (F₁₉₈ - c)³

h_{ROT} = rule of thumb bandwidth = 103.795 (in thousands)

Standard Error in *italics*.

Table A5. Estimates for the Overall Effect of SIMPLES - "Correction" Of the Measurement Error -1996/97

Dependent Variable								
Change in the Number of Employees between 1996 and 1997								
	Bandwidth (in thousands)							
Regressor: Simples (T ₁₉₇)	300		200		50		h _{ROT}	
	Without Corr.	“Corrected”	Without Corr.	“Corrected”	Without Corr.	“Corrected”	Without Corr.	“Corrected”
Linear	1.69 <i>0.80</i>	1.69 <i>0.80</i>	1.64 <i>0.79</i>	1.64 <i>0.79</i>	1.80 <i>1.20</i>	1.79 <i>1.20</i>	1.77 <i>0.95</i>	1.77 <i>0.95</i>
Quadratic	1.87 <i>0.81</i>	1.87 <i>0.81</i>	1.57 <i>0.81</i>	1.58 <i>0.81</i>	1.80 <i>1.20</i>	1.60 <i>1.31</i>	1.59 <i>0.96</i>	1.60 <i>0.96</i>
Cubic	1.94 <i>0.85</i>	1.96 <i>0.84</i>	1.49 <i>0.79</i>	1.54 <i>0.79</i>	1.59 <i>1.31</i>	1.77 <i>1.31</i>	1.60 <i>0.98</i>	1.61 <i>0.97</i>
Number of Observations	3499		2312		603		1022	
Note: Included as dummy regressors for subsectors of the industry, states, single establishment and age. Linear Specification uses: ΔF ₉₆ e Di ₉₇ · (F ₁₉₆ - c) Quadratic Specification uses: (F ₁₉₆ - c) ² - (F ₁₉₅ - c) ² e Di ₉₇ · (F ₉₆ - c) ² Quadratic Specification uses: (F ₁₉₆ - c) ³ - (F ₁₉₅ - c) ³ e Di ₉₇ · (F ₉₆ - c) ³ “Corrected” = Changing (T ₁₉₇ =1 & D ₁₉₇ =0) to D ₉₇ =1 and fixing these to F ₁₉₆ =720000 h _{ROT} = rule of thumb bandwidth = 84.453 (in thousands) Standard Error in <i>italics</i> .								

Table A6. Estimates for the Overall Effect of SIMPLES - "Correction" Of the Measurement Error -1998/99

Dependent Variable								
Change in the Number of Employees between 1998 and 1999								
	Bandwidth (in thousands)							
Regressor: Simples (T ₁₉₉)	300		200		50		h _{ROT}	
	Without Corr.	“Corrected”	Without Corr.	“Corrected”	Without Corr.	“Corrected”	Without Corr.	“Corrected”
Linear	2.77 <i>1.34</i>	2.77 <i>1.34</i>	2.82 <i>1.15</i>	2.86 <i>1.15</i>	2.52 <i>2.11</i>	2.52 <i>2.11</i>	0.72 <i>1.18</i>	0.73 <i>1.18</i>
Quadratic	2.54 <i>1.24</i>	2.62 <i>1.24</i>	2.80 <i>1.08</i>	2.82 <i>1.08</i>	2.70 <i>2.11</i>	2.68 <i>2.11</i>	0.93 <i>1.20</i>	0.94 <i>1.19</i>
Cubic	2.64 <i>1.22</i>	2.63 <i>1.23</i>	2.67 <i>1.06</i>	2.66 <i>1.05</i>	2.63 <i>2.12</i>	2.63 <i>2.11</i>	1.18 <i>1.20</i>	1.21 <i>1.19</i>
Number of Observations	2111		1394		393		794	
Note: Included as dummy regressors for subsectors of the industry, states, single establishment and age. Linear Specification uses: ΔF ₁₉₈ e Di ₁₉₉ · (F ₁₉₈ – c) Quadratic Specification uses: (F ₁₉₈ – c) ² – (F ₁₉₇ – c) ² e Di ₁₉₉ · (F ₁₉₈ – c) ² Quadratic Specification uses: (F ₁₉₈ – c) ³ – (F ₁₉₇ – c) ³ e Di ₁₉₉ · (F ₁₉₈ – c) ³ “Corrected” = Changing (T ₁₉₉ =1 & D ₁₉₉ =0) to D ₁₉₉ =1 and fixing these to F ₁₉₈ =1200000 h _{ROT} = rule of thumb bandwidth = 103.795 (in thousands) Standard Error in <i>italics</i> .								